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Mr. Samuel H. Scudder has prepared²⁰ a table for the determination of the genera of North American Melanopli, and gives notes on the distribution of species.

Part V of the Bibliography of American Entomology prepared by the U. S. Division of Entomology has recently been issued. It contains 1679 entries. We are glad to note that Dr. Howard expresses his intention to bring the work down to date, and to continue it hereafter. It is extremely serviceable to all working entomologists.

At a meeting of the Birmingham Entomological Society²¹ Mr. W. Wilkinson exhibited a collection of insects made in the Madeira and Canary Islands in February, March and April, 1896. Among the most interesting species were *Pararge xiphia* from Madeira and *P. xiphiodes* from Grand Canary, two closely allied but distinct species peculiar to the Madeiras and Canaries respectively. Mr. Kenricks said that the most curious feature in the Canary Island fauna was the occurrence of American forms. *Vanessa huntera* occurs only in the Canaries and America, and *Anosia plexippus* (= *Danaïs archippus*) which is common in the Canaries is also an American species. Mr. Bethune Baker said that in working on the moths he noticed American affinities in several groups, especially in the genus *Phlogophora*, which was much more closely allied to American than to European forms.

PSYCHOLOGY.¹

Mr. Spencer's Psychology.—The following letter by Professor J. Mark Baldwin, President of the American Psychological Association, was read before the Philosophical Club of Bryn Mawr College, on the occasion of the celebration of the completion of Mr. Spencer's "Synthetic Philosophy."

Princeton, March 9.

To the Club:

In speaking briefly of Mr. Spencer's psychology, perhaps I can do no better than throw the impressions which I have into the form of informal pros and cons. I should premise what I have to say, however, with the remark that one of my reasons for not accepting your kind

²⁰ Proc. Amer. Acad. Arts and Sci., XXXII, No. 9.

²¹ Entomologist, XXX, 93, 94.

¹ Edited by H. C. Warren, Princeton University, Princeton, N. J.

invitation to be present and speak on this occasion, was that I could not just now find time to put in exact form such a detailed appreciation as the proper attitude toward so great a subject requires. Yet I feel unwilling to allow the occasion to pass without bringing a trifle of some kind to add to your fuller tribute to Mr. Spencer. I beg, therefore, that you will consider what I say as impressions left on my mind from the study of Mr. Spencer's psychology—my personal reaction to his work—rather than as a well-formed opinion which I should in any way wish to commend to others.

First, then, for the pros.

1. Of course, the great and evident service rendered by Mr. Spencer in this department, as in others, has been his deliberate and argued advocacy of evolution. In all the spheres of the application of evolution doctrine, there was a prejudice to overcome; in none, such prejudice as here. It is not overcome yet. Spencer's is to-day, the name to refute, to pulverize, to anathematize, to ridicule, by the opposition which in Darwin's case spoke through the Bishop of Oxford, and which has used Spencer for its fulcrum ever since in raising the resistance with which science loads the other end of the lever. Fire a gun at the "First Principles," put to flight feelings and representative feelings, and re-representative feelings, and the cosmos is safe. In all this Spencer has borne the brunt. But all the while Herbart and Wundt and James—may the last-mentioned forgive me, but he more than others has ridden rough-shod over the pages of Spencer—have been getting the credit which they deserve for the coming of a naturalistic era in psychology.

In this matter of naturalism, our ship has had to change her course one hundred and eighty degrees; Spencer set the compass true in the new direction, and through all the buffetings, and breastings, and poundings, and creakings we are only just now getting her head to bear after his compass.

2. It is to me also a great thing that Mr. Spencer did not draw too sharp a line between biological and psychological evolution. All the talk about the boundary lines of science, the divisions of this *Gebiet* from that, this "point of view" here and that there—all this to the contrary, the objective science of mind is practically the great science after all. Of course, lots of qualifications are necessary here, and philosophers will demur, but I for one feel somewhat more secure when I have behind me the methods of objective science. Darwin's way of studying the emotions was more fruitful than that of his predecessors. Our knowledge of memory has been most advanced by research in

pathology and brain localization. Once discover pain-nerves, and we refute a theory academic from the year I. Now the credit of taking this objective point of view generally and of using so deliberately biological data and even biological explanations belongs to Spencer. What is the use trying to complete a psychology simply as such? What is the good trying with Wundt to abstract "pure feeling" from "pure sensation" when really both are pure mythology? Isn't it the defect of biology also that it tries too much to complete a biology merely as such, without the help of psychology? When two sciences are ripe enough to fall together and be one, that is good; but there is no earthly use in trying to keep them as far as possible apart in the meantime. In this, I think, Spencer was right. There is only one evolution, let us keep an eye on both sides of it.

3. As to Mr. Spencer's positive contributions to psychology, these I may not discuss in detail. They are mainly incidental to the ideas in the service of which his speculations were made. His theories have nearly all been disproved; I mean his particular theories. But his contributions by the way are of very great importance. And even the disproved theories, they have been leading-strings for thought and motives for research to countless workers. You cannot open a competent book in any of three or four great departments of thought, but you find the most fruitful discussions turning about the hypotheses of Spencer. I take it that this is one of the greatest possible services of a great man—to produce definitely directed effort, even though his private views go down in the result.

And now for the cons.

Here what there is to say seems to me to be mainly a statement of the limitations incident to the very qualities which we have found to be Mr. Spencer's principal claim to our admiration. Every great idea seems in its first blush simpler than it is. Natural selection, for example, is proving itself by giving ground. But the fame of its author, Darwin, does not suffer from that, even apart from the fact that Darwin was wiser than are his disciples. We are now saying "back to Darwin," and although we can never say "back to Spencer," yet Spencer has his place fixed for all that. The real limitations of Spencer are evident just in this contrast with Darwin.

1. Spencer's genetic Psychology was an idea, just as his genetic Biology and Sociology were ideas, and the same idea. But he could not prove this idea in all these departments. He could only see the evident and surface facts which his idea was likely to explain. This he did in a very remarkable way in the "System of Synthetic Philoso-

phy," the completion of which you are celebrating to-day. It is marvelous that a single mind should have been able to make so many happy hits in so rapid and, in a good sense, superficial survey of all these fields. But it was, I think, rather that he had a stupendously great idea than that he had a stupendously great mind. He was armed with the thought which all the natural sciences are tending to prove true; but the same sciences are showing that almost all the ways in which he took this idea to work were not true. This means that Mr. Spencer's personal tendencies were in the direction of his gifts, toward a deductive, hypothetical, inexact way of treating scientific details.

2. Then as to his method, that too is a great limitation. It has always seemed to me that Mr. Spencer was a great example of the costliness of analogy. Analogy, analogy everywhere! It is not a part of the interconnection of the sciences that the facts of one should be explained by analogies from another. Yet such a procedure Spencer constantly falls into. Chemical analogies in biology, biological analogies in psychology and sociology, mechanical analogies—dissolutions, integrations, etc., of force—all the way through. In psychology this is especially deplorable, since it leads to a general tendency—also apparent in the sociology—to be satisfied with inadequate analysis; and inasmuch as the analogies are drawn from spheres of simpler activities, it is just the refinements which characterize the higher as higher that escape it. Everybody knows the flat sterility which results when the association theory is applied to the higher reaches of thought and conduct. It is like proving a bed of tulips to be mere onions by going through them and nipping off all the blooms. So to solve the problems of psychology by biological or chemical analogies, is to make use of a weapon which figuratively speaking nips off all the blooms. But this is only part of another and deeper limitation, to wit:

3. Mr. Spencer's view of evolution is not what we are coming to-day to consider the true thought of natural genesis. Herein is the real and essential limitation of Spencer's work considered from a philosophical point of view—and possibly I am departing from the topic assigned me in mentioning it. He believes, I think, that the new not only comes out of the old, but that it is explained by the full statement of the old. Now this is a philosophy; and it is a leveling-down philosophy—whatever we say to the question as to where it finally lands us. It tends to state the tulip in terms of its roots. Now this is all right as science, but when it is made a philosophy and a presupposition to science, then it is baleful. For besides rendering it excessively difficult to be a good scientist—not to judge it as a philosophy—it makes the thinker liable

to continual "illusions of simplicity"—(thus to designate the fallacy of taking things to be too simple). So with Mr. Spencer's psychology: it impresses one as a series of great illusions of simplicity. Many of his generalizations depend each upon just one fact of striking and easy interpretation from his point of view. The "surplus energy theory" of play, the "dream theory" of spirit, the "dance theory," the "vocal theory." And many of the more important principles which are not of so easy an interpretation seem nevertheless to owe their place as corner stones in the system to this same tendency to simplification. Such are "utility," "use inheritance," etc.

4. The same thing is seen in the ease with which difficult places are glossed over. A bridge of analogy or often of mere vagueness of expression covers a yawning gap sometimes at a most critical place. This, however, is so common a criticism of Mr. Spencer, that I need not take it farther.

In conclusion I may say that the balance to the good in any fair estimate of Mr. Spencer's work is so enormous, that we should not hesitate to recognize as correct the verdict of all the world to the effect that he is one of the main factors in the main movement in the history of modern thought.—J. MARK BALDWIN.

Involuntary Movements.—The manner in which ideas present in consciousness determine involuntary movements, especially of the hands, has already received considerable experimental attention from Lehmann, Fère and Jastrow. Their results have lately been supplemented in two important particulars by Mr. M. A. Tucker, of Stanford University.² The object of Mr. Tucker's investigation was to determine, first, any general tendencies to motion in the hand, apart from the spatial influence of thought; and second, the comparative value of these involuntary movements in adults and children. The apparatus used was similar in its essential features to Jastrow's automograph. To prevent the attention taking on a directional character, in the experiments where this was to be avoided, the subject recited the multiplication table, conjugated French verbs, etc.

As regards the first point of investigation, there was found to be a "tendency for the hands and arms resting in front of the body to move inward toward the median plane of the body." There did not appear to be any necessary tendency for the hands to move toward a visible object to which the attention was directed, if that object was thought of simply as at rest; but the sight of moving objects, or the remem-

² Amer. Jour. of Psychol., 1897, VIII, 394-404.

brance of them, caused an involuntary imitation of the direction of the moving stimuli, not only by the hands, but also by the whole body; this tendency manifested itself in a distinctly observable swaying of the head. As to the second point, the investigation brought out the general fact that "children are governed by and subject to the same laws as adults, but to a less extent." Individual variations were wider in them than in adults. No differences were found in children due to age or sex.

These experiments seem to substantiate the views of Féré and Lehmann, while they disagree with those of Jastrow, who reported a tendency of the hands to move toward stationary objects whenever the attention was directed towards their locality.

Contraction of the Field of Vision.—It has been questioned for sometime whether the contraction of the field of vision after repeated tests of the periphery (attributed to fatigue) is limited strictly to pathological cases. Of recent years it has been generally assumed that this phenomenon appears frequently in normal life as well, and in consequence it has lost much of its former value as a psychiatric symptom. Dr. Erdmann Mueller, however, has recently published³ the results of an investigation undertaken upon 102 normal subjects which show the contrary to be true. The tests were made without a perimeter, the object of vision being a small, white ivory ball of the size of a cherry-pit, which was fixed to a thin, translucent piece of fish-bone.

Out of the entire number of subjects tested, but two showed any unusual contraction of the visual field. In three separate trials apiece, with each eye, these two subjects showed a contraction varying from 10° to 30° in the course of a single trial, but each showed no contraction whatever in one of the tests. The subjects in question were found to be suffering from slight neurasthenia, but exhibited no severe neurotic symptoms whatever. Of the remaining 100 subjects, three showed a contraction of 5° , and thirty a contraction of from 2° – 4° only; but in seventeen of these cases a contraction of the field on one side was accompanied by a widening on the other, so that the apparent change may easily have been due to a slight shifting of the position of the head during the progress of the experiment. The other sixty-seven subjects showed a contraction of 1° or less, which was below the threshold of experimental precision.

As a result of these tests, Dr. Mueller concludes that the contraction occurs among healthy persons, if at all, only in the very slightest de-

³ Arch. f. Psychiat. u. Nervenkr., 1896, XXIX, 225–230.

gree; where it is observed to any marked extent among the apparently healthy, it is shown to be accompanied by other neurotic symptoms. The test requires considerable attention on the part of the subject, and an apparent contraction of the field on the first trial often proves to be due to a lack of adjustment of the attention. The existence of genuine contraction can be affirmed only after repeated trials when the subject has become accustomed to the methods and has learned to maintain his attention properly.—H. C. W.

PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Zoological Society of London.—The sixty-eighth anniversary meeting of this Society was held at their offices, 3 Hanover Square, W. In the absence of the President, the Chair was taken by Dr. Edward Hamilton, Vice-President, who was supported by Lord Medway, Sir Hugh Low, General Trevor, Dr. Henry Woodward, F. R. S., Lt.-Col. H. H. Godwin-Austen, F. R. S., and many other Fellows of the Society.

After the Auditors Report had been read, and a vote of thanks accorded to them, and some other preliminary business had been transacted, the Report of the Council on the proceedings of the Society during the past year was read by Mr. P. L. Sclater, F. R. S., the Secretary. It stated that the number of Fellows on the 1st of January, 1897, was 3,098, showing a net increase of 71 members during the year 1896. The number of new Fellows that joined the Society in 1896 was 207, which was the largest number of elections that had taken place in any year since 1877.

The total receipts of the Society for 1896 had amounted to £27,081.10.4, which was £123.1.3 more than the very successful year 1895. The ordinary expenditure in 1896 had amounted to £23,788.1.2, which was an increase of £327.4.4 over that of the year 1895. Besides this a sum of £2617.15.0 had been paid and charged to extraordinary expenditure, of which amount £2600 had been paid on account of the construction of the new house for ostriches and cranes. A further sum of £1000 had also been transferred to the Deposit Account, leaving a balance of £1066.15.4 to be carried forward for the benefit of the present year. The usual scientific meetings had been held during the year 1896, and a large number of valuable communications had been received upon every branch of zoology. These had been published in the annual volume of "Proceedings," which contained 1110 pages, illustrated by 52